THE AETIOLOGY OF HYSTERIA

(1896)

Sigmund Freud
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EDITOR'S NOTE

ZUR ÄTIOLOGIE DER HYSTERIE

(*a*) GERMAN EDITIONS:

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1906 *S.K.S.N.*, **1**, 149–180. (1911, 2nd ed.; 1920, 3rd ed.; 1922, 4th ed.)
1925 *G.S.*, **1**, 404–38.
1952 *G.W.*, **1**, 425–59.

(*b*) ENGLISH TRANSLATIONS:

'The Aetiology of Hysteria'
1924 *C.P.*, **1**, 183–219. (Tr. C. M. Baines.)

Included (No. XXXVI) in Freud's own collection of abstracts of his early works (1897*b*). The present translation is a modified version of that of 1924.

According to a footnote in the *Wiener klinische Rundschau* of May 31, 1896, this paper is based on a lecture delivered by Freud before the 'Verein für Psychiatrie und Neurologie' on May 2. The correctness of this date is, however, questionable. In an unpublished letter to Fliess of Thursday, April 16, Freud wrote that on the following Tuesday [April 21] he was due to give a lecture before the 'Psychiatrischer Verein'. He does not specify the topic, but in another unpublished letter, dated April 26 and 28, 1896, he reported having given a lecture before that society on the aetiology of hysteria. He went on to remark that 'the donkeys gave it an icy reception' and that Krafft-Ebing, who was in the chair, said it sounded like a scientific fairy tale. In yet another letter, dated May 30, this time included in the Fliess correspondence (Freud, 1950*a*, Letter 46), he wrote: 'In defiance of my colleagues, I have written out my lecture on the aetiology of hysteria in full for Paschkis [the

editor of the *Rundschau*].' And its publication in fact started in that paper on the next day. It seems to follow from all this that the lecture was actually given on April 21, 1896.

The present work may be regarded as an amplified repetition of the first section of its predecessor, the second paper on the neuro-psychoses of defence (1896*b*). Freud's findings on the causes of hysteria are given in fuller detail, with some account of the difficulties he had to overcome in reaching them. Much more space is given, especially in the last part of the paper, to the sexual experiences in childhood which Freud believed to lie behind the later symptoms. As in the previous papers, these experiences are regarded as invariably initiated by adults: the realization of the existence of infantile sexuality still lay in the future. There is nevertheless a hint (on pp. 214–15) of what was to be described in the *Three Essays* (1905*d*), *Standard Ed.*, 7, 191, as the 'polymorphously perverse' character of infantile sexuality. Among other points of interest, we may note an increasing tendency to prefer psychological to neurological explanations (p. 203), and an early attempt at solving the problem of 'choice of neurosis' (pp. 219–20), which was to be a constantly returning subject of discussion. Freud's varying views on this are traced in the Editor's Note to 'The Disposition to Obsessional Neurosis' (1913*i*), *Standard Ed.*, 12, 313 ff.; the subject had in fact been approached already in the two papers preceding the present one (pp. 156 and 168–9 above).

Freud's Standard Edition.

THE AETIOLOGY OF HYSTERIA

[I]

GENTLEMEN,—When we set out to form an opinion about the causation of a pathological state such as hysteria, we begin by adopting the method of anamnestic investigation: we question the patient or those about him in order to find out to what harmful influences they themselves attribute his having fallen ill and developed these neurotic symptoms. What we discover in this way is, of course, falsified by all the factors which commonly hide the knowledge of his own state from a patient —by his lack of scientific understanding of aetiological influences, by the fallacy of *post hoc, propter hoc*, by his reluctance to think about or mention certain noxae and traumas. Thus in making an anamnestic investigation of this sort, we keep to the principle of not adopting the patients' belief without a thorough critical examination, of not allowing them to lay down our scientific opinion for us on the aetiology of the neurosis. Although we do, on the one hand, acknowledge the truth of certain constantly repeated assertions, such as that the hysterical state is a long-persisting after-effect of an emotion experienced in the past, we have, on the other hand, introduced into the aetiology of hysteria a factor which the patient himself never brings forward and whose validity he only reluctantly admits —namely, the hereditary disposition derived from his progenitors. As you know, in the view of the influential school of Charcot heredity alone deserves to be recognized as the true cause of hysteria, while all other noxae of the most various nature and intensity only play the part of incidental causes, of '*agents provocateurs*'.

You will readily admit that it would be a good thing to have a second method of arriving at the aetiology of hysteria, one in which we should feel less dependent on the assertions of the patients themselves. A dermatologist, for instance, is able to recognize a sore as luetic from the character of its margins, of the crust on it and of its shape, without being misled by the

protestations of his patient, who denies any source of infection for it; and a forensic physician can arrive at the cause of an injury, even if he has to do without any information from the injured person. In hysteria, too, there exists a similar possibility of penetrating from the symptoms to a knowledge of their causes. But in order to explain the relationship between the method which we have to employ for this purpose and the older method of anamnestic enquiry, I should like to bring before you an analogy taken from an advance that has in fact been made in another field of work.

Imagine that an explorer arrives in a little-known region where his interest is aroused by an expanse of ruins, with remains of walls, fragments of columns, and tablets with half-effaced and unreadable inscriptions. He may content himself with inspecting what lies exposed to view, with questioning the inhabitants—perhaps semi-barbaric people—who live in the vicinity, about what tradition tells them of the history and meaning of these archaeological remains, and with noting down what they tell him—and he may then proceed on his journey. But he may act differently. He may have brought picks, shovels and spades with him, and he may set the inhabitants to work with these implements. Together with them he may start upon the ruins, clear away the rubbish, and, beginning from the visible remains, uncover what is buried. If his work is crowned with success, the discoveries are self-explanatory: the ruined walls are part of the ramparts of a palace or a treasure-house; the fragments of columns can be filled out into a temple; the numerous inscriptions, which, by good luck, may be bilingual, reveal an alphabet and a language, and, when they have been deciphered and translated, yield undreamed-of information about the events of the remote past, to commemorate which the monuments were built. *Saxa loquuntur!*[1]

If we try, in an approximately similar way, to induce the symptoms of a hysteria to make themselves heard as witnesses to the history of the origin of the illness, we must take our start from Josef Breuer's momentous discovery: *the symptoms of hysteria* (apart from the stigmata[2]) *are determined by certain*

[1] ['Stones talk!']

[2] [These, defined by Charcot (1887, 255) as 'the permanent symptoms of hysteria', had been described by Freud as non-psychogenic in *Studies on Hysteria* (1895*d*), *Standard Ed.*, 2, 265. They had also been discussed by Breuer, ibid., 244 f. (See also ibid., 15.) In a very early letter to Breuer of June 29, 1892, Freud (1941*a*) had described the origin of these symptoms as 'highly obscure'.]

experiences of the patient's which have operated in a traumatic fashion and which are being reproduced in his psychical life in the form of mnemic symbols.[1] What we have to do is to apply Breuer's method —or one which is essentially the same—so as to lead the patient's attention back from his symptom to the scene in which and through which that symptom arose; and, having thus located the scene, we remove the symptom by bringing about, during the reproduction of the traumatic scene, a subsequent correction of the psychical course of events which took place at the time.

It is no part of my intention to-day to discuss the difficult technique of this therapeutic procedure or the psychological discoveries which have been obtained by its means. I have been obliged to start from this point only because the analyses conducted on Breuer's lines seem at the same time to open up the path to the causes of hysteria. If we subject a fairly large number of symptoms in a great number of subjects to such an analysis, we shall, of course, arrive at a knowledge of a correspondingly large number of traumatically operative scenes. It was in these experiences that the efficient causes of hysteria came into action. Hence we may hope to discover from the study of these traumatic scenes what the influences are which produce hysterical symptoms and in what way they do so.

This expectation proves true; and it cannot fail to, since Breuer's theses, when put to the test in a considerable number of cases, have turned out to be correct. But the path from the symptoms of hysteria to its aetiology is more laborious and leads through other connections than one would have imagined.

For let us be clear on this point. Tracing a hysterical symptom back to a traumatic scene assists our understanding only if the scene satisfies two conditions; if it possesses the relevant *suitability to serve as a determinant* and if it recognizably possesses the necessary *traumatic force*. Instead of a verbal explanation, here is an example. Let us suppose that the symptom under consideration is hysterical vomiting; in that case we shall feel that

[1] [See footnote 3, above, p. 49.]

we have been able to understand its causation (except for a certain residue) if the analysis traces the symptom back to an experience which *justifiably produced a high amount of disgust*—for instance, the sight of a decomposing dead body. But if, instead of this, the analysis shows us that the vomiting arose from a great fright, e.g. from a railway accident, we shall feel dissatisfied and will have to ask ourselves how it is that the fright has led to the particular symptom of vomiting. This derivation lacks *suitability as a determinant*. We shall have another instance of an insufficient explanation if the vomiting is supposed to have arisen from, let us say, eating a fruit which had partly gone bad. Here, it is true, the vomiting *is* determined by disgust, but we cannot understand how, in this instance, the disgust could have become so powerful as to be perpetuated in a hysterical symptom; the experience lacks *traumatic force*.

Let us now consider how far the traumatic scenes of hysteria which are uncovered by analysis fulfil, in a fairly large number of symptoms and cases, the two requirements which I have named. Here we meet with our first great disappointment. It is true, indeed, that the traumatic scene in which the symptom originated does in fact occasionally possess both the qualities —suitability as a determinant and traumatic force—which we require for an understanding of the symptom. But far more frequently, incomparably more frequently, we find one of the three other possibilities realized, which are so unfavourable to an understanding. Either the scene to which we are led by analysis and in which the symptom first appeared seems to us unsuited for determining the symptom, in that its content bears no relation to the nature of the symptom; or the allegedly traumatic experience, though it *does* have a relation to the symptom, proves to be an impression which is normally innocuous and incapable as a rule of producing any effect; or, lastly, the 'traumatic scene' leaves us in the lurch in both respects, appearing at once innocuous and unrelated to the character of the hysterical symptom.

(Here I may remark in passing that Breuer's view of the origin of hysterical symptoms is not shaken by the discovery of traumatic scenes which correspond to experiences that are insignificant in themselves. For Breuer assumed—following Charcot—that even an innocuous experience can be heightened into a trauma and can develop determining force if it happens to the subject when he is in a special psychical condition—in what is described as a *hypnoid state*.[1] I find, however, that there are often no grounds whatever for presupposing the presence of such hypnoid states. What remains decisive is that the theory of hypnoid states contributes nothing to the solution of the other difficulties, namely that the traumatic scenes so often lack suitability as determinants.)

Moreover, Gentlemen, this first disappointment we meet with in following Breuer's method is immediately succeeded by another, and one that must be especially painful to us as physicians. When our procedure leads, as in the cases described above, to findings which are insufficient as an explanation both in respect to their suitability as determinants and to their traumatic effectiveness, we also fail to secure any therapeutic gain; the patient retains his symptoms unaltered, in spite of the initial result yielded by the analysis. You can understand how great the temptation is at this point to proceed no further with what is in any case a laborious piece of work.

But perhaps all we need is a new idea in order to help us out of our dilemma and lead to valuable results. The idea is this. As we know from Breuer, hysterical symptoms can be resolved if, starting from them, we are able to find the path back to the memory of a traumatic experience. If the memory which we have uncovered does not answer our expectations, it may be that we ought to pursue the same path a little further; perhaps behind the first traumatic scene there may be concealed the memory of a second, which satisfies our requirements better and whose reproduction has a greater therapeutic effect; so that the scene that was first discovered only has the significance of a connecting link in the chain of associations. And perhaps this situation may repeat itself; inoperative scenes may be interpolated more than once, as necessary transitions in the process of reproduction, until we finally make our way from the hysterical symptom to the scene which is really operative traumatically and which is satisfactory in every respect, both therapeutically and analytically. Well, Gentlemen, this supposition is correct. If the first-discovered scene is unsatisfactory, we tell our patient that this experience explains nothing, but that behind it there

[1] [See p. 46 above.]

must be hidden a more significant, earlier, experience; and we direct his attention by the same technique to the associative thread which connects the two memories—the one that has been discovered and the one that has still to be discovered.[1] A continuation of the analysis then leads in every instance to the reproduction of new scenes of the character we expect. For example, let us take once again the case of hysterical vomiting which I selected before, and in which the analysis first led back to a fright from a railway accident—a scene which lacked suitability as a determinant. Further analysis showed that this accident had aroused in the patient the memory of another, earlier accident, which, it is true, he had not himself experienced but which had been the occasion of his having a ghastly and revolting sight of a dead body. It is as though the combined operation of the two scenes made the fulfilment of our postulates possible, the one experience supplying, through fright, the traumatic force and the other, from its content, the determining effect. The other case, in which the vomiting was traced back to eating an apple which had partly gone bad, was amplified by the analysis somewhat in the following way. The bad apple reminded the patient of an earlier experience: while he was picking up windfalls in an orchard he had accidentally come upon a dead animal in a revolting state.

I shall not return any further to these examples, for I have to confess that they are not derived from any case in my experience but are inventions of mine. Most probably, too, they are bad inventions. I even regard such solutions of hysterical symptoms as impossible. But I was obliged to make up fictitious examples for several reasons, one of which I can state at once. The real examples are all incomparably more complicated: to relate a single one of them in detail would occupy the whole period of this lecture. The chain of associations always has more than two links; and the traumatic scenes do not form a simple row, like a string of pearls, but ramify and are interconnected like genealogical trees, so that in any new experience two or

[1] I purposely leave out of this discussion the question of what the category is to which the association between the two memories belongs (whether it is an association by simultaneity, or by causal connection, or by similarity of content), and of what psychological character is to be attributed to the various 'memories' (conscious or unconscious).

more earlier ones come into operation as memories. In short, giving an account of the resolution of a single symptom would in fact amount to the task of relating an entire case history.

But we must not fail to lay special emphasis on one conclusion to which analytic work along these chains of memory has unexpectedly led. We have learned that *no hysterical symptom can arise from a real experience alone, but that in every case the memory of earlier experiences awakened in association to it plays a part in causing the symptom.* If—as I believe—this proposition holds good *without exception,* it furthermore shows us the basis on which a psychological theory of hysteria must be built.

You might suppose that the rare instances in which analysis is able to trace the symptom back direct to a traumatic scene that is thoroughly suitable as a determinant and possesses traumatic force, and is able, by thus tracing it back, at the same time to remove it (in the way described in Breuer's case history of Anna O.[1])—you might suppose that such instances must, after all, constitute powerful objections to the general validity of the proposition I have just put forward. It certainly looks so. But I must assure you that I have the best grounds for assuming that even in such instances there exists a chain of operative memories which stretches far back behind the first traumatic scene, *even though* the reproduction of the latter alone may have the result of removing the symptom.

It seems to me really astonishing that hysterical symptoms can only arise with the co-operation of memories, especially when we reflect that, according to the unanimous accounts of the patients themselves, these memories did not come into their consciousness at the moment when the symptom first made its appearance. Here is much food for thought; but these problems must not distract us at this point from our discussion of the aetiology of hysteria.[2] We must rather ask ourselves: where shall we get to if we follow the chains of associated memories which the analysis has uncovered? How far do they extend? Do they come anywhere to a natural end? Do they perhaps lead to experiences which are in some way alike, either in their

[1] [See p. 29 f. above.]

[2] [Freud takes up the postponed problem below, on p. 212 ff. He had already touched on it in a footnote to Section I of his second paper on the neuro-psychoses of defence (1896*b*), pp. 166–7 *n.* above.]

content or the time of life at which they occur, so that we may discern in these universally similar factors the aetiology of hysteria of which we are in search?

The knowledge I have so far gained already enables me to answer these questions. If we take a case which presents several symptoms, we arrive by means of the analysis, starting from each symptom, at a series of experiences the memories of which are linked together in association. To begin with, the chains of memories lead backwards separately from one another; but, as I have said, they ramify. From a single scene two or more memories are reached at the same time, and from these again side-chains proceed whose individual links may once more be associatively connected with links belonging to the main chain. Indeed, a comparison with the genealogical tree of a family whose members have also intermarried, is not at all a bad one. Other complications in the linkage of the chains arise from the circumstance that a single scene may be called up several times in the same chain, so that it has multiple relationships to a later scene, and exhibits both a direct connection with it and a connection established through intermediate links. In short, the concatenation is far from being a simple one; and the fact that the scenes are uncovered in a reversed chronological order (a fact which justifies our comparison of the work with the excavation of a stratified ruined site) certainly contributes nothing to a more rapid understanding of what has taken place.

If the analysis is carried further, new complications arise. The associative chains belonging to the different symptoms begin to enter into relation with one another; the genealogical trees become intertwined. Thus a particular symptom in, for instance, the chain of memories relating to the symptom of vomiting, calls up not only the earlier links in its own chain but also a memory from another chain, relating to another symptom, such as a headache. This experience accordingly belongs to both series, and in this way it constitutes a *nodal point*[1] Several such nodal points are to be found in every

[1] ['Nodal points' had been described by Freud in his technical contribution to *Studies on Hysteria* (1895*d*), *Standard Ed.*, **2**, 290 and 295. It is worth comparing the account of the chains of associations given there (ibid., 288 ff.) with the present much shorter one. An actual instance of a 'nodal point'—the word 'wet'—is described in the analysis of the first dream in the 'Dora' case history (1905*e*), ibid., **7**, 90 f.]

analysis. Their correlate in the clinical picture may perhaps be that from a certain time onwards both symptoms have appeared together, symbiotically, without in fact having any internal dependence on each other. Going still further back, we come upon nodal points of a different kind. Here the separate associative chains converge.[1] We find experiences from which two or more symptoms have proceeded; one chain has attached itself to one detail of the scene, the second chain to another detail.

But the most important finding that is arrived at if an analysis is thus consistently pursued is this. Whatever case and whatever symptom we take as our point of departure, *in the end we infallibly come to the field of sexual experience.* So here for the first time we seem to have discovered an aetiological precondition for hysterical symptoms.

From previous experience I can foresee that it is precisely against this assertion or against its universal validity that your contradiction, Gentlemen, will be directed. Perhaps it would be better to say, your *inclination* to contradict; for none of you, no doubt, have as yet any investigations at your disposal which, based upon the same procedure, might have yielded a different result. As regards the controversial matter itself, I will only remark that the singling out of the sexual factor in the aetiology of hysteria springs at least from no preconceived opinion on my part. The two investigators as whose pupil I began my studies of hysteria, Charcot and Breuer, were far from having any such presupposition; in fact they had a personal disinclination to it which I originally shared. Only the most laborious and detailed investigations have converted me, and that slowly enough, to the view I hold to-day. If you submit my assertion that the aetiology of hysteria lies in sexual life to the strictest examination, you will find that it is supported by the fact that in some eighteen[2] cases of hysteria I have been able to discover this connection in every single symptom, and, where the circumstances allowed, to confirm it by therapeutic success. No

[1] [The regular occurrence first of divergence and then of convergence in chains of associations is also mentioned by Freud as characteristic of dream-analysis. Cf. Section II of 'Remarks on the Theory and Practice of Dream Interpretation' (1923*c*), *Standard Ed.*, **19**, 110.]

[2] [Cf. the earlier figure of thirteen (pp. 152 and 163 above.)]

doubt you may raise the objection that the nineteenth or the twentieth analysis will perhaps show that hysterical symptoms are derived from other sources as well, and thus reduce the universal validity of the sexual aetiology to one of eighty per cent. By all means let us wait and see; but, since these eighteen cases are at the same time *all* the cases on which I have been able to carry out the work of analysis and since they were not picked out by anyone for my convenience, you will find it understandable that I do not share such an expectation but am prepared to let my belief run ahead of the evidential force of the observations I have so far made. Besides, I am influenced by another motive as well, which for the moment is of merely subjective value. In the sole attempt to explain the physiological and psychical mechanism of hysteria which I have been able to make in order to correlate my observations, I have come to regard the participation of sexual motive forces as an indispensable premiss.

Eventually, then, after the chains of memories have converged, we come to the field of sexuality and to a small number of experiences which occur for the most part at the same period of life—namely, at puberty. It is in these experiences, it seems, that we are to look for the aetiology of hysteria, and through them that we are to learn to understand the origin of hysterical symptoms. But here we meet with a fresh disappointment and a very serious one. It is true that these experiences, which have been discovered with so much trouble and extracted out of all the mnemic material, and which seemed to be the ultimate traumatic experiences, have in common the two characteristics of being sexual and of occurring at puberty; but in every other respect they are very different from each other both in *kind* and in *importance*. In some cases, no doubt, we are concerned with experiences which must be regarded as severe traumas—an attempted rape, perhaps, which reveals to the immature girl at a blow all the brutality of sexual desire, or the involuntary witnessing of sexual acts between parents, which at one and the same time uncovers unsuspected ugliness and wounds childish and moral sensibilities alike, and so on. But in other cases the experiences are astonishingly trivial. In one of my women patients it turned out that her neurosis was based on the experience of a boy of her acquaintance stroking her hand tenderly

and, at another time, pressing his knee against her dress as they sat side by side at table, while his expression let her see that he was doing something forbidden. For another young lady, simply hearing a riddle which suggested an obscene answer had been enough to provoke the first anxiety attack and with it to start the illness. Such findings are clearly not favourable to an understanding of the causation of hysterical symptoms. If serious and trifling events alike, and if not only experiences affecting the subject's own body but visual impressions too and information received through the ears are to be recognized as the ultimate traumas of hysteria, then we may be tempted to hazard the explanation that hysterics are peculiarly constituted creatures—probably on account of some hereditary disposition or degenerative atrophy—in whom a shrinking from sexuality, which normally plays some part at puberty, is raised to a pathological pitch and is permanently retained; that they are, as it were, people who are psychically inadequate to meeting the demands of sexuality. This view, of course, leaves hysteria in men out of account. But even without blatant objections such as that, we should scarcely be tempted to be satisfied with this solution. We are only too distinctly conscious of an intellectual sense of something half-understood, unclear and insufficient.

Luckily for our explanation, some of these sexual experiences at puberty exhibit a further inadequacy, which is calculated to stimulate us into continuing our analytic work. For it sometimes happens that they, too, lack suitability as determinants—although this is much more rarely so than with the traumatic scenes belonging to later life. Thus, for instance, let us take the two women patients whom I have just spoken of as cases in which the experiences at puberty were actually innocent ones. As a result of those experiences the patients had become subject to peculiar painful sensations in the genitals which had established themselves as the main symptoms of the neurosis. I was unable to find indications that they had been determined either by the scenes at puberty or by later scenes; but they were certainly not normal organic sensations nor signs of sexual excitement. It seemed an obvious thing, then, to say to ourselves that we must look for the determinants of these symptoms in yet other experiences, in experiences which went still further

back—and that we must, for the second time, follow the saving notion which had earlier led us from the first traumatic scenes to the chains of memories behind them. In doing so, to be sure, we arrive at the period of earliest childhood, a period before the development of sexual life; and this would seem to involve the abandonment of a sexual aetiology. But have we not a right to assume that even the age of childhood is not wanting in slight sexual excitations, that later sexual development may perhaps be decisively influenced by childhood experiences? Injuries sustained by an organ which is as yet immature, or by a function which is in process of developing, often cause more severe and lasting effects than they could do in maturer years. Perhaps the abnormal reaction to sexual impressions which surprises us in hysterical subjects at the age of puberty is quite generally based on sexual experiences of this sort in childhood, in which case those experiences must be of a similar nature to one another, and must be of an important kind. If this is so, the prospect is opened up that what has hitherto had to be laid at the door of a still unexplained hereditary predisposition may be accounted for as having been acquired at an early age. And since infantile experiences with a sexual content could after all only exert a psychical effect through their *memory-traces*, would not this view be a welcome amplification of the finding of psycho-analysis which tells *us that hysterical symptoms can only arise with the co-operation of memories*? [P. 197 above.]

II

You will no doubt have guessed, Gentlemen, that I should not have carried this last line of thought so far if I had not wanted to prepare you for the idea that it is this line alone which, after so many delays, will lead us to our goal. For now we are really at the end of our wearisome and laborious analytic work, and here we find the fulfilment of all the claims and expectations upon which we have so far insisted. If we have the perseverance to press on with the analysis into early childhood, as far back as a human memory is capable of reaching, we invariably bring the patient to reproduce experiences which, on account both of their peculiar features and of their relations to

the symptoms of his later illness, must be regarded as the aetiology of his neurosis for which we have been looking. These *infantile* experiences are once more *sexual* in content, but they are of a far more uniform kind than the scenes at puberty that had been discovered earlier. It is now no longer a question of sexual topics having been aroused by some sense impression or other, but of sexual experiences affecting the subject's own body —of *sexual intercourse* (in the wider sense). You will admit that the *importance* of such scenes needs no further proof; to this may now be added that, in every instance, you will be able to discover in the details of the scenes the *determining* factors which you may have found lacking in the other scenes—the scenes which occurred later and were reproduced earlier. [Cf. p. 193.]

I therefore put forward the thesis that at the bottom of every case of hysteria there are *one or more occurrences of premature sexual experience*, occurrences which belong to the earliest years of childhood but which can be reproduced through the work of psycho-analysis in spite of the intervening decades.[1] I believe that this is an important finding, the discovery of a *caput Nili*[2] in neuropathology; but I hardly know what to take as a starting-point for a continuation of my discussion of this subject. Shall I put before you the actual material I have obtained from my analyses? Or shall I rather try first to meet the mass of objections and doubts which, as I am surely correct in supposing, have now taken possession of your attention? I shall choose the latter course; perhaps we shall then be able to go over the facts more calmly.

(*a*) No one who is altogether opposed to a psychological view of hysteria, who is unwilling to give up the hope that some day it will be possible to trace back its symptoms to 'finer anatomical changes' and who has rejected the view that the material foundations of hysterical changes are bound to be of the same kind as those of our normal mental processes—no one who adopts this attitude will, of course, put any faith in the results of our analyses; however, the difference in principle between his premisses and ours absolves us from the obligation of convincing him on individual points.

[1] (*Footnote added* 1924:) Cf. my remark on p. 204.

[2] [Source of the Nile.]

But other people, too, although they may be less averse to psychological theories of hysteria, will be tempted, when considering our analytic findings, to ask what degree of certainty the application of psycho-analysis offers. Is it not very possible either that the physician forces such scenes upon his docile patients, alleging that they are memories, or else that the patients tell the physician things which they have deliberately invented or have imagined and that he accepts those things as true? Well, my answer to this is that the general doubt about the reliability of the psycho-analytic method can be appraised and removed only when a complete presentation of its technique and results is available. Doubts about the genuineness of the infantile sexual scenes can, however, be deprived of their force here and now by more than one argument. In the first place, the behaviour of patients while they are reproducing these infantile experiences is in every respect incompatible with the assumption that the scenes are anything else than a reality which is being felt with distress and reproduced with the greatest reluctance. Before they come for analysis the patients know nothing about these scenes. They are indignant as a rule if we warn them that such scenes are going to emerge. Only the strongest compulsion of the treatment can induce them to embark on a reproduction of them. While they are recalling these infantile experiences to consciousness, they suffer under the most violent sensations, of which they are ashamed and which they try to conceal; and, even after they have gone through them once more in such a convincing manner, they still attempt to withhold belief from them, by emphasizing the fact that, unlike what happens in the case of other forgotten material, they have no feeling of remembering the scenes.[1]

This latter piece of behaviour seems to provide conclusive proof. Why should patients assure me so emphatically of their unbelief, if what they want to discredit is something which—from whatever motive—they themselves have invented?

It is less easy to refute the idea that the doctor forces reminiscences of this sort on the patient, that he influences him

[1] (*Footnote added* 1924:) All this is true; but it must be remembered that at the time I wrote it I had not yet freed myself from my *overvaluation* of reality and my *low valuation* of phantasy. [Cf. the additional footnote to the previous paper, p. 168 above.]

by suggestion to imagine and reproduce them. Nevertheless it appears to me equally untenable. I have never yet succeeded in forcing on a patient a scene I was expecting to find, in such a way that he seemed to be living through it with all the appropriate feelings. Perhaps others may be more successful in this.

There are, however, a whole number of other things that vouch for the reality of infantile sexual scenes. In the first place there is the uniformity which they exhibit in certain details, which is a necessary consequence if the preconditions of these experiences are always of the same kind, but which would otherwise lead us to believe that there were secret understandings between the various patients. In the second place, patients sometimes describe as harmless events whose significance they obviously do not understand, since they would be bound otherwise to be horrified by them. Or again, they mention details, without laying any stress on them, which only someone of experience in life can understand and appreciate as subtle traits of reality.

Events of this sort strengthen our impression that the patients must really have experienced what they reproduce under the compulsion of analysis as scenes from their childhood. But another and stronger proof of this is furnished by the relationship of the infantile scenes to the content of the whole of the rest of the case history. It is exactly like putting together a child's picture-puzzle: after many attempts, we become absolutely certain in the end which piece belongs in the empty gap; for only that one piece fills out the picture and at the same time allows its irregular edges to be fitted into the edges of the other pieces in such a manner as to leave no free space and to entail no overlapping.[1] In the same way, the contents of the infantile scenes turn out to be indispensable supplements to the associative and logical framework of the neurosis, whose insertion makes its course of development for the first time evident, or even, as we might often say, self-evident.

Without wishing to lay special stress on the point, I will add

[1] [This analogy was used by Freud again more than a quarter of a century later, in a period when 'jig-saw' puzzles had become an adult pastime. See 'Remarks on the Theory and Practice of Dream Interpretation' (1923*c*), *Standard Ed.*, **19**, 116.]

that in a number of cases therapeutic evidence of the genuineness of the infantile scenes can also be brought forward. There are cases in which a complete or partial cure can be obtained without our having to go as deep as the infantile experiences. And there are others in which no success at all is obtained until the analysis has come to its natural end with the uncovering of the earliest traumas. In the former cases we are not, I believe, secure against relapses; and my expectation is that a complete psycho-analysis implies a radical cure of the hysteria.[1] We must not, however, be led into forestalling the lessons of observation.

There would be one other proof, and a really unassailable one, of the genuineness of childhood sexual experiences—namely, if the statements of someone who is being analysed were to be confirmed by someone else, whether under treatment or not. These two people will have had to have taken part in the same experience in their childhood—perhaps to have stood in some sexual relationship to each other. Such relations between children are, as you will hear in a moment [p. 208], by no means rare. Moreover, it quite often happens that both of those concerned subsequently fall ill of neuroses; yet I regard it as a fortunate accident that, out of eighteen cases, I have been able to obtain an objective confirmation of this sort in two. In one instance, it was the brother (who had remained well) who of his own accord confirmed—not, it is true, his earliest sexual experiences with his sister (who was the patient)—but at least scenes of that kind from later childhood, and the fact that there had been sexual relations dating further back. In the other instance, it happened that two women whom I was treating had as children had sexual relations with the same man, in the course of which certain scenes had taken place *à trois*. A particular symptom, which was derived from these childhood events, had developed in both women, as evidence of what they had experienced in common.

(*b*) Sexual experiences in childhood consisting in stimulation of the genitals, coitus-like acts, and so on, must therefore be

[1] [It is of interest to compare this with the less sanguine views expressed by Freud in his very late paper, 'Analysis Terminable and Interminable' (1937*c*).]

recognized, in the last analysis, as being the traumas which lead to a hysterical reaction to events at puberty and to the development of hysterical symptoms. This statement is certain to be met from different directions by two mutually contradictory objections. Some people will say that sexual abuses of this kind, whether practised upon children or between them, happen too seldom for it to be possible to regard them as the determinant of such a common neurosis as hysteria. Others will perhaps argue that, on the contrary, such experiences are very frequent—much too frequent for us to be able to attribute an aetiological significance to the fact of their occurrence. They will further maintain that it is easy, by making a few enquiries, to find people who remember scenes of sexual seduction and sexual abuse in their childhood years, and yet who have never been hysterical. Finally we shall be told, as a weighty argument, that in the lower strata of the population hysteria is certainly no more common than in the highest ones, whereas everything goes to show that the injunction for the sexual safeguarding of childhood is far more frequently transgressed in the case of the children of the proletariat.

Let us begin our defence with the easier part of the task. It seems to me certain that our children are far more often exposed to sexual assaults than the few precautions taken by parents in this connection would lead us to expect. When I first made enquiries about what was known on the subject, I learnt from colleagues that there are several publications by paediatricians which stigmatize the frequency of sexual practices by nurses and nursery maids, carried out even on infants in arms; and in the last few weeks I have come across a discussion of 'Coitus in Childhood' by Dr. Stekel (1895)[1] in Vienna. I have not had time to collect other published evidence; but even if it were only scanty, it is to be expected that increased attention to the subject will very soon confirm the great frequency of sexual experiences and sexual activity in childhood.

Lastly, the findings of my analysis are in a position to speak for themselves. In all eighteen cases (cases of pure hysteria and of hysteria combined with obsessions, and comprising six men

[1] [This date is wrongly given as '1896' in the German editions. Incidentally, Stekel only came to hear of Freud some five years after this. Cf. Jones, 1955, 8.]

and twelve women) I have, as I have said, come to learn of sexual experiences of this kind in childhood. I can divide my cases into three groups, according to the origin of the sexual stimulation. In the first group it is a question of assaults—of single, or at any rate isolated, instances of abuse, mostly practised on female children, by adults who were strangers, and who, incidentally, knew how to avoid inflicting gross, mechanical injury. In these assaults there was no question of the child's consent, and the first effect of the experience was preponderantly one of fright. The second group consists of the much more numerous cases in which some adult looking after the child—a nursery maid or governess or tutor, or, unhappily all too often, a close relative[1]—has initiated the child into sexual intercourse and has maintained a regular love relationship with it—a love relationship, moreover, with its mental side developed—which has often lasted for years. The third group, finally, contains child-relationships proper—sexual relations between two children of different sexes, mostly a brother and sister, which are often prolonged beyond puberty and which have the most far-reaching consequences for the pair. In most of my cases I found that two or more of these aetiologies were in operation together; in a few instances the accumulation of sexual experiences coming from different quarters was truly amazing. You will easily understand this peculiar feature of my observations, however, when you consider that the patients I was treating were all cases of severe neurotic illness which threatened to make life impossible.

Where there had been a relation between two children I was sometimes able to prove that the boy—who, here too, played the part of the aggressor—had previously been seduced by an adult of the female sex, and that afterwards, under the pressure of his prematurely awakened libido and compelled by his memory, he tried to repeat with the little girl exactly the same practices that he had learned from the adult woman, without making any modification of his own in the character of the sexual activity.

In view of this, I am inclined to suppose that children cannot find their way to acts of sexual aggression unless they have been seduced previously. The foundation for a neurosis would

[1] [Cf. an Editor's footnote to the previous paper, p. 164 above.]

accordingly always be laid in childhood by adults, and the children themselves would transfer to one another the disposition to fall ill of hysteria later. I will ask you to consider a moment longer the special frequency with which sexual relations in childhood occur precisely between brothers and sisters and cousins, as a result of their opportunities for being together so often; supposing, then, ten or fifteen years later several members of the younger generation of the family are found to be ill, might not this appearance of a family neurosis naturally lead to the false supposition that a hereditary disposition is present where there is only a *pseudo-heredity* and where in fact what has taken place is a handing-on, an infection in childhood?

Now let us turn to the other objection [p. 207], which is based precisely on an acknowledgement of the frequency of infantile sexual experiences and on the observed fact that many people who remember scenes of that kind have *not* become hysterics. Our first reply is that the excessive frequency of an aetiological factor cannot possibly be used as an objection to its aetiological significance.[1] Is not the tubercle bacillus ubiquitous and is it not inhaled by far more people than are found to fall ill of tuberculosis? And is its aetiological significance impaired by the fact that other factors must obviously be at work too before the tuberculosis, which is its specific effect, can be evoked? In order to establish the bacillus as the specific aetiology it is enough to show that tuberculosis cannot possibly occur without its playing a part. The same doubtless applies to our problem. It does not matter if many people experience infantile sexual scenes without becoming hysterics, provided only that all the people who become hysterics have experienced scenes of that kind. The area of occurrence of an aetiological factor may be freely allowed to be wider than that of its effect, but it must not be narrower. Not everyone who touches or comes near a smallpox patient develops smallpox; nevertheless infection from a smallpox patient is almost the only known aetiology of the disease.

It is true that if infantile sexual activity were an almost

[1] [The whole question of 'specific aetiology' had been discussed by Freud more fully in his second paper on anxiety neurosis (1895*f*), p. 135 ff. above.]

universal occurrence the demonstration of its presence in every case would carry no weight. But, to begin with, to assert such a thing would certainly be a gross exaggeration; and secondly, the aetiological pretensions of the infantile scenes rest not only on the regularity of their appearance in the anamneses of hysterics, but, above all, on the evidence of there being associative and logical ties between those scenes and the hysterical symptoms—evidence which, if you were given the complete history of a case, would be as clear as daylight to you.

What can the other factors be which the 'specific aetiology' of hysteria still needs in order actually to produce the neurosis? That, Gentlemen, is a theme in itself, which I do not propose to enter upon. To-day I need only indicate the point of contact at which the two parts of the topic—the specific and the auxiliary aetiology—fit into one another. No doubt a considerable quantity of factors will have to be taken into account. There will be the subject's inherited and personal constitution, the inherent importance of the infantile sexual experiences, and, above all, their number: a brief relationship with a strange boy, who afterwards becomes indifferent, will leave a less powerful effect on a girl than intimate sexual relations of several years' standing with her own brother. In the aetiology of the neuroses quantitative preconditions are as important as qualitative ones: there are threshold-values which have to be crossed before the illness can become manifest. Moreover, I do not myself regard this aetiological series as complete; nor does it solve the riddle of why hysteria is not more common among the lower classes.[1] (You will remember, by the way, what a surprisingly large incidence of hysteria was reported by Charcot among working-class *men*). I may also remind you that a few years ago I myself pointed out a factor, hitherto little considered, to which I attribute the leading role in provoking hysteria after puberty. I then[2] put forward the view that the outbreak of hysteria may almost invariably be traced to a *psychical conflict* arising through an incompatible idea setting in action a *defence* on the part of the ego and calling up a demand

[1] [Cf. above, p. 207.]

[2] [See Freud's first paper on the neuro-psychoses of defence (1894*a*), p. 47 f. above.]

for repression. What the circumstances are in which a defensive endeavour of this kind has the pathological effect of actually thrusting the memory which is distressing to the ego into the unconscious and of creating a hysterical symptom in its place I was not able to say at that time. But to-day I can repair the omission. *The defence achieves its purpose of thrusting the incompatible idea out of consciousness if there are infantile sexual scenes present in the (hitherto normal) subject in the form of unconscious memories, and if the idea that is to be repressed can be brought into logical or associative connection with an infantile experience of that kind.*

Since the ego's efforts at defence depend upon the subject's total moral and intellectual development, the fact that hysteria is so much rarer in the lower classes than its specific aetiology would warrant is no longer entirely incomprehensible.

Let us return once again, Gentlemen, to the last group of objections, the answering of which has led us such a long way. We have heard and have acknowledged that there are numerous people who have a very clear recollection of infantile sexual experiences and who nevertheless do not suffer from hysteria. This objection has no weight; but it provides an occasion for making a valuable comment. According to our understanding of the neurosis, people of this kind *ought* not to be hysterical at all, or at any rate, not hysterical as a result of the scenes which they consciously remember. With our patients, those memories are never conscious; but we cure them of their hysteria by transforming their unconscious memories of the infantile scenes into conscious ones. There was nothing that we could have done or needed to do about the fact that they have had such experiences. From this you will perceive that the matter is not merely one of the existence of the sexual experiences, but that a psychological precondition enters in as well. The scenes must be present as *unconscious memories*; only so long as, and in so far as, they are unconscious are they able to create and maintain hysterical symptoms. But what decides whether those experiences produce conscious or unconscious memories—whether that is conditioned by the content of the experiences, or by the time at which they occur, or by later influences—that is a fresh problem, which we shall prudently avoid. Let me merely remind you that, as its first conclusion, analysis has arrived

at the proposition that *hysterical symptoms are derivatives of memories which are operating unconsciously.*

(*c*) Our view then is that infantile sexual experiences are the fundamental precondition for hysteria, are, as it were, the *disposition* for it and that it is they which create the hysterical symptoms, but that they do not do so immediately, but remain without effect to begin with and only exercise a pathogenic action later, when they have been aroused after puberty in the form of unconscious memories. If we maintain this view, we shall have to come to terms with the numerous observations which show that a hysterical illness may already make its appearance in childhood and before puberty. This difficulty, however, is cleared up as soon as we examine more closely the data gathered from analyses concerning the chronology of the infantile experiences. We then learn that in our severe cases the formation of hysterical symptoms begins—not in exceptional instances, but, rather, as a regular thing—at the age of eight, and that the sexual experiences which show no immediate effect invariably date further back, into the third or fourth, or even the second year of life. Since in no single instance does the chain of effective experiences[1] break off at the age of eight, I must assume that this time of life, the period of growth in which the second dentition takes place, forms a boundary line for hysteria, after which the illness cannot be caused. From then on, a person who has not had sexual experiences earlier can no longer become disposed to hysteria; and a person who *has* had experiences earlier, is already able to develop hysterical symptoms. Isolated instances of the occurrence of hysteria on the other side of this boundary line (that is, *before* the age of eight) may be interpreted as a phenomenon of precocious maturity. The existence of this boundary-line is very probably connected with developmental processes in the sexual system. Precocity of somatic sexual development may often be observed, and it is even possible that it can be promoted by too early sexual stimulation.[2]

[1] [The sense would appear to suggest that this is a condensed way of saying 'experiences that might be expected to be effective'.]

[2] [Cf. the long footnote in Section I of the previous paper (1896*b*), pp. 166–7 above.]

In this way we obtain an indication that a certain *infantile* state of the psychical functions, as well as of the sexual system, is required in order that a sexual experience occurring during this period shall later on, in the form of a memory, produce a pathogenic effect. I do not venture as yet, however, to make any more precise statement on the nature of this psychical infantilism or on its chronological limits.

(*d*) Another objection might arise from exception being taken to the supposition that the *memory* of infantile sexual experiences produces such an enormous pathogenic effect, while the actual experience itself has none. And it is true that we are not accustomed to the notion of powers emanating from a mnemic image which were absent from the real impression. You will moreover notice the consistency with which the proposition that symptoms can only proceed from memories is carried through in hysteria. None of the later scenes, in which the symptoms arise, are the effective ones; and the experiences which *are* effective have at first no result. But here we are faced with a problem which we may very justifiably keep separate from our theme. It is true that we feel impelled to make a synthesis, when we survey the number of striking conditions that we have come to know: the fact that in order to form a hysterical symptom a defensive effort against a distressing idea must be present, that this idea must exhibit a logical or associative connection with an unconscious memory through a few or many intermediate links, which themselves, too, remain unconscious at the moment, that this unconscious memory must have a sexual content, that its content must be an experience which occurred during a certain infantile period of life. It is true that we cannot help asking ourselves how it comes about that this memory of an experience that was innocuous at the time it happened, should posthumously produce the abnormal effect of leading a psychical process like defence to a pathological result, while it itself remains unconscious.

But we shall have to tell ourselves that this is a purely psychological problem, whose solution may perhaps necessitate certain hypotheses about normal psychical processes and about the part played in them by consciousness, but that this problem

may be allowed to remain unsolved for the time being, without detracting from the value of the insight we have so far gained into the aetiology of hysterical phenomena.

III

Gentlemen, the problem, the approaches to which I have just formulated, concerns the *mechanism* of the formation of hysterical symptoms. We find ourselves obliged, however, to describe the *causation* of those symptoms without taking that mechanism into account, and this involves an inevitable loss of completeness and clarity in our discussion. Let us go back to the part played by the infantile sexual scenes. I am afraid that I may have misled you into over-estimating their power to form symptoms. Let me, therefore, once more stress the fact that every case of hysteria exhibits symptoms which are determined, not by infantile but by later, often by recent, experiences. Other symptoms, it is true, go back to the very earliest experiences and belong, so to speak, to the most ancient nobility. Among these latter are above all to be found the numerous and diverse sensations and paraesthesias of the genital organs and other parts of the body, these sensations and paraesthesias being phenomena which simply correspond to the sensory content of the infantile scenes, reproduced in a hallucinatory fashion, often painfully intensified.

Another set of exceedingly common hysterical phenomena—painful need to urinate, the sensation accompanying defaecation, intestinal disturbances, choking and vomiting, indigestion and disgust at food—were also shown in my analyses (and with surprising regularity) to be derivatives of the same childhood experiences and were explained without difficulty by certain invariable peculiarities of those experiences. For the idea of these infantile sexual scenes is very repellent to the feelings of a sexually normal individual; they include all the abuses known to debauched and impotent persons, among whom the buccal cavity and the rectum are misused for sexual purposes. For physicians, astonishment at this soon gives way to a complete understanding. People who have no hesitation in satisfying their sexual desires upon children cannot be expected to jib at finer shades in the methods of obtaining that satisfaction; and

the sexual impotence which is inherent in children inevitably forces them into the same substitutive actions as those to which adults descend if they become impotent. All the singular conditions under which the ill-matched pair conduct their love-relations—on the one hand the adult, who cannot escape his share in the mutual dependence necessarily entailed by a sexual relationship, and who is yet armed with complete authority and the right to punish, and can exchange the one role for the other to the uninhibited satisfaction of his moods, and on the other hand the child, who in his helplessness is at the mercy of this arbitrary will, who is prematurely aroused to every kind of sensibility and exposed to every sort of disappointment, and whose performance of the sexual activities assigned to him is often interrupted by his imperfect control of his natural needs—all these grotesque and yet tragic incongruities reveal themselves as stamped upon the later development of the individual and of his neurosis, in countless permanent effects which deserve to be traced in the greatest detail. Where the relation is between two children, the character of the sexual scenes is none the less of the same repulsive sort, since every such relationship between children postulates a previous seduction of one of them by an adult. The psychical consequences of these child-relations are quite extraordinarily far-reaching; the two individuals remain linked by an invisible bond throughout the whole of their lives.

Sometimes it is the accidental circumstances of these infantile sexual scenes which in later years acquire a determining power over the symptoms of the neurosis. Thus, in one of my cases the circumstance that the child was required to stimulate the genitals of a grown-up woman with his foot was enough to fixate his neurotic attention for years on to his legs and to their function, and finally to produce a hysterical paraplegia. In another case, a woman patient suffering from anxiety attacks which tended to come on at certain hours of the day could not be calmed unless a particular one of her many sisters stayed by her side all the time. Why this was so would have remained a riddle if analysis had not shown that the man who had committed the assaults on her used to enquire at every visit whether this sister, who he was afraid might interrupt him, was at home.

It may happen that the determining power of the infantile scenes is so much concealed that, in a superficial analysis, it is bound to be overlooked. In such instances we imagine that we have found the explanation of some particular symptom in the content of one of the later scenes—until, in the course of our work, we come upon the same content in one of the *infantile* scenes, so that in the end we are obliged to recognize that, after all, the later scene only owes its power of determining symptoms to its agreement with the earlier one. I do not wish because of this to represent the later scene as being unimportant; if it was my task to put before you the rules that govern the formation of hysterical symptoms, I should have to include as one of them that the idea which is selected for the production of a symptom is one which has been called up by a combination of several factors and which has been aroused from various directions simultaneously. I have elsewhere tried to express this in the formula: *hysterical symptoms are overdetermined.*[1]

One thing more, Gentlemen. It is true that earlier [p. 213 f.] I put the relation between recent and infantile aetiology aside as a separate theme. Nevertheless, I cannot leave the subject without overstepping this resolution at least with one remark. You will agree with me that there is *one* fact above all which leads us astray in the psychological understanding of hysterical phenomena, and which seems to warn us against measuring psychical acts in hysterics and in normal people with the same yardstick. That fact is the discrepancy between psychically exciting stimuli and psychical reactions which we come upon in hysterical subjects. We try to account for it by assuming the presence in them of a general abnormal sensitivity to stimuli, and we often endeavour to explain it on a physiological basis, as if in such patients certain organs of the brain which serve to transmit stimuli were in a peculiar chemical state (like the spinal centres of a frog, perhaps, which has been injected with strychnine) or as if these cerebral organs had withdrawn from the influence of higher inhibiting centres (as in animals being experimented on under vivisection). Occasionally one or other of these concepts may be perfectly valid as an explanation of hysterical phenomena; I do not dispute this. But the main part

[1] [See Freud's technical contribution to *Studies on Hysteria* (1895*d*), *Standard Ed.*, 2, 263 and 290.]

of the phenomenon—of the abnormal, exaggerated, hysterical reaction to psychical stimuli—admits of another explanation, an explanation which is supported by countless examples from the analyses of patients. And this is as follows: *The reaction of hysterics is only apparently exaggerated; it is bound to appear exaggerated to us because we only know a small part of the motives from which it arises.*

In reality, this reaction is proportionate to the exciting stimulus; thus it is normal and psychologically understandable. We see this at once when the analysis has added to the manifest motives, of which the patient is conscious, those other motives, which have been operative without his knowing about them, so that he could not tell us of them.

I could spend hours demonstrating the validity of this important assertion for the whole range of psychical activity in hysteria, but I must confine myself here to a few examples. You will remember the mental 'sensitiveness' which is so frequent among hysterical patients and which leads them to react to the least sign of being depreciated as though they had received a deadly insult. What would you think, now, if you were to observe this high degree of readiness to feel hurt on the slightest occasion, if you came across it between two normal people, a husband and wife, perhaps? You would certainly infer that the conjugal scene you had witnessed was not solely the result of this latest trifling occasion, but that inflammable material had been piling up for a long time and that the whole heap of it had been set alight by the final provocation.

I would ask you to carry this line of thought over on to hysterical patients. It is not the latest slight—which, in itself, is minimal—that produces the fit of crying, the outburst of despair or the attempt at suicide, in disregard of the axiom that an effect must be proportionate to its cause; the small slight of the present moment has aroused and set working the memories of very many, more intense, earlier slights, behind all of which there lies in addition the memory of a serious slight in childhood which has never been overcome. Or again, let us take the instance of a young girl who blames herself most frightfully for having allowed a boy to stroke her hand in secret, and who from that time on has been overtaken by a neurosis. You can, of course, answer the puzzle by pronouncing

her an abnormal, eccentrically disposed and over-sensitive person; but you will think differently when analysis shows you that the touching of her hand reminded her of another, similar touching, which had happened very early in her childhood and which formed part of a less innocent whole, so that her self-reproaches were actually reproaches about that old occasion. Finally, the problem of the hysterogenic points[1] is of the same kind. If you touch a particular spot, you do something you did not intend: you awaken a memory which may start off a convulsive attack, and since you know nothing of this psychical intermediate link you refer the attack directly to the operation of your touch. The patients are in the same state of ignorance and therefore fall into similar errors. They constantly establish 'false connections'[2] between the most recent cause, which they are conscious of, and the effect, which depends on so many intermediate links. If, however, the physician has been able to bring together the conscious and unconscious motives for the purpose of explaining a hysterical reaction, he is almost always obliged to recognize that the seemingly exaggerated reaction is appropriate and is abnormal only in its form.

You may, however, rightly object to this justification of the hysterical reaction to psychical stimuli and say that nevertheless the reaction is not a normal one. For why do healthy people behave differently? Why do not all *their* excitations of long ago come into operation once more when a new, present-day, excitation takes place? One has an impression, indeed, that with hysterical patients it is as if all their old experiences—to which they have already reacted so often and, moreover, so violently—had retained their effective power; as if such people were incapable of disposing of their psychical stimuli. Quite true, Gentlemen, something of the sort must really be assumed. You must not forget that in hysterical people when there is a present-day precipitating cause, the old experiences come into operation in the form of *unconscious memories*. It looks as though the difficulty of disposing of a present impression, the im-

[1] [The term used by Charcot, e.g. 1887, 85 ff., is 'hysterogenic zones'; but the present word appears also in Freud's contribution to *Studies on Hysteria*, *Standard Ed.*, **2**, 261.]

[2] [See Section II of the first paper on the neuro-psychoses of defence (1894*a*), p. 52 above.]

possibility of transforming it into a powerless memory, is attached precisely to the character of the psychical unconscious.[1] You see that the remainder of the problem lies once more in the field of psychology—and, what is more, a psychology of a kind for which philosophers have done little to prepare the way for us.

To this psychology, which has yet to be created to meet our needs—to this future *psychology of the neuroses*—I must also refer you when, in conclusion, I tell you something which will at first make you afraid that it may disturb our dawning comprehension of the aetiology of hysteria. For I must affirm that the aetiological role of infantile sexual experience is not confined to hysteria but holds good equally for the remarkable neurosis of obsessions, and perhaps also, indeed, for the various forms of chronic paranoia and other functional psychoses. I express myself on this with less definiteness, because I have as yet analysed far fewer cases of obsessional neurosis than of hysteria; and as regards paranoia, I have at my disposal only a single full analysis and a few fragmentary ones. But what I discovered in these cases seemed to be reliable and filled me with confident expectations for other cases. You will perhaps remember that already, at an earlier date,[2] I recommended that hysteria and obsessions should be grouped together under the name of '*neuroses of defence*', even before I had come to know of their common infantile aetiology. I must now add that—although this need not be expected to happen in general—every one of my cases of obsessions revealed a substratum of hysterical symptoms,[3] mostly sensations and pains, which went back precisely to the earliest childhood experiences. What, then, determines whether the infantile sexual scenes which have remained unconscious will later on, when the other pathogenic factors are super-added, give rise to hysterical or to obsessional neurosis or even to paranoia? This increase in our knowledge seems, as you see, to prejudice the aetiological value of these scenes, since it removes the specificity of the aetiological relation.

[1] [This is an early hint of Freud's later view of the 'timelessness' of the unconscious. See Section V of the metapsychological paper on 'The Unconscious' (1915*e*), *Standard Ed.*, **14**, 187 *n.*]

[2] [I.e. in the first paper on the neuro-psychoses of defence (1894*a*).]

[3] [See p. 168 above.]

I am not yet in a position, Gentlemen, to give a reliable answer to this question. The number of cases I have analysed is not large enough nor have the determining factors in them been sufficiently various. So far, I have observed that obsessions can be regularly shown by analysis to be disguised and transformed *self-reproaches about acts of sexual aggression in childhood*, and are therefore more often met with in men than in women, and that men develop obsessions more often than hysteria. From this I might conclude that the character of the infantile scenes—whether they were experienced with pleasure or only passively—has a determining influence on the choice of the later neurosis; but I do not want to underestimate the significance of the age at which these childhood actions occur, and other factors as well. Only a discussion of further analyses can throw light on these points. But when it becomes clear which are the decisive factors in the choice between the possible forms of the neuro-psychoses of defence, the question of what the mechanism is in virtue of which that particular form takes shape will once again be a purely psychological problem.

I have now come to the end of what I have to say to-day. Prepared as I am to meet with contradiction and disbelief, I should like to say one thing more in support of my position. Whatever you may think about the conclusions I have come to, I must ask you not to regard them as the fruit of idle speculation. They are based on a laborious individual examination of patients which has in most cases taken up a hundred or more hours of work. What is even more important to me than the value you put on my results is the attention you give to the procedure I have employed. This procedure is new and difficult to handle, but it is nevertheless irreplaceable for scientific and therapeutic purposes. You will realize, I am sure, that one cannot properly deny the findings which follow from this modification of Breuer's procedure so long as one puts it aside and uses only the customary method of questioning patients. To do so would be like trying to refute the findings of histological technique by relying upon macroscopic examination. The new method of research gives wide access to a new element in the psychical field of events, namely, to processes of thought which have remained unconscious—which, to use Breuer's expression,

are 'inadmissible to consciousness'.[1] Thus it inspires us with the hope of a new and better understanding of all functional psychical disturbances. I cannot believe that psychiatry will long hold back from making use of this new pathway to knowledge.

[1] [See Part 5 of Breuer's theoretical contribution to *Studies on Hysteria* (1895*d*), *Standard Ed.*, **2**, 225.]